

On Observations of the Satellites of Uranus. By A. Marth.

(Communicated by W. Lassell, Esq.)

I should like you to mention at the meeting of the Royal Astronomical Society, that during the present appearance of *Uranus* observers may again have a chance of contributing something towards the settlement of the question respecting Herschel's additional satellites, if they will take the trouble of making careful eye-drafts of the stars in the neighbourhood, and along the track of the planet on January 28 and March 2 and 3, 1875, and on preceding and following nights. The evidence of these sketches will not bear so directly on the question at issue as that of sketches made last winter on the nights, mentioned in my letter in the *Monthly Notices* for December 1873, would have done, yet it will be of some service, especially if it should turn out that the opportunities of last winter have been everywhere neglected.

If I had got a fair opportunity, I should have settled the question some years ago, and, I trust, in a satisfactory manner. But the attempts which I repeatedly made, had to be made in such an unsuitable atmosphere, and under such distressing circumstances, that I had to give them up as futile and hopeless. The Washington observers ought now to give their help, and to furnish the requisite evidence from the heavens, so that a critical summing up of the arguments pro and con, may ensure a satisfactory verdict.

It is a great pleasure, that at last, after a long interval, we have got again new series of observations of the satellites of the outer planets, without which the older series could not be made properly available. Should I be in a position to be able to afford the time, I shall not fail to publish in future regularly Ephemerides, so that observers may know beforehand what to look for. But for the purpose I must first get hold of the new observations, and the requisite time for correcting my old elements of the satellites' orbits.

Note on a Discussion relating to the Rejection of Discordant Observations. By E. J. Stone, M.A. F.R.S.

I am afraid that we are sometimes more sensitive to the tone of criticism of our work than to the tone of our criticism of the work of others. I can, therefore, only venture to ask anyone interested in the discussion which has taken place to read over, in order of publication, my original paper in the *Monthly Notices*, April 1868, to understand what I professed to do, and I believe did, in that paper; then Mr. Glaisher's animadversions upon that paper in the *Notices* for April 1873, and my reply: and I would call especial attention to the statement of my supposed

views, interpolated with free comments on page 398 of Mr. Glaisher's paper.

I should be most reluctant to enter upon mere verbal criticism; but if Mr. Glaisher does attach any importance to the statement towards the close of his short note in the *Monthly Notices* for March, that "I never held that the h of every observation was 'à priori' equally likely to have any value from 0 to ∞ , as I took the usual result as a first approximation." I would remark that the point of the method, developed at considerable length in the *Notices* for April 1873, was to give to the h of every observation the value, whatever that might be, which belonged to it from the run of the numbers in a proposed case. Upon this point, to show that I have not misrepresented Mr. Glaisher, I give three extracts from his paper. On page 394 we read: "The law (of facility) with h determined separately for each observation, can be made to represent the facts with a near approximation to accuracy."

On page 395, speaking of the arithmetical mean, "instead of saying after the application of the rule, 'This is the final result,' I say, No! this is not good enough for a final result; but it is good enough to weight the observations from, and thence a better result is obtained and so on. Of course, if the first approximation is very nearly the best that the observations will give, the process of weighting will alter it very slightly." Again, on page 397, "An anomalous observation would obtain a very small weight, but never none at all, and this is as it should be." Other extracts might be given. It is true that Mr. Glaisher attempted to solve the question by successive approximations, but if the process had been properly conducted, the results obtained must have finally tended, without limit, to those obtained by any correct direct process. I cannot, therefore, see the force of the remark, that the arithmetical mean was taken as a first approximation. It might shorten or lengthen the work according as the arithmetical mean was, or was not, a close approximation to the final result. More than this it could not do. At least such is my opinion, and will, I venture to believe, be the opinion of mathematicians generally.

Royal Observatory, Cape of Good Hope,
1874, June 14.

The present number contains a Lithographic Plate of Drawings of Coggia's Comet, by Mr. Plummer, communicated by him to the Society at the November meeting.

ERRATUM.

Page 27, lines 4 to 7. *The sentence which follows should have been within marks of quotation, as part of the quotation from Sir W. Herschel:—*

Most likely this satellite also was seen among the supposed satellites south of the planet, March 27, 1794, where we find mention made 'of some others south at a good distance.' In that case this will make a second observation.